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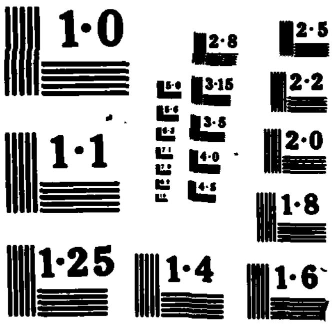
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The optimist fell from a ten story window;
As he passed each bar,
He shouted to his friends inside,
"I'm alright so far."

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PREFACE

This paper was prepared as a contribution to *Government Policies for Industrial Innovation: Design, Implementation, Evaluation* (New York: Associated Faculty Press, 1984), edited by Prof. J. David Roessner and sponsored by the Policy Studies Organization. It represents a major revision of my earlier Rand Paper (P-6919), *The Evaluation of Innovation,* in terms of clearing up earlier ambiguities of concept and definition and including significant amounts of new material.

EVALUATING PROGRAM INNOVATION: A POLICY PERSPECTIVE

Introduction

Like our proverbial optimist, the innovator--be it an individual or an organization--is ever sanguine that this particular innovation is (or shortly will be) successful. The conditions which motivate the innovation--some sort of recognized and important shortcoming or opportunity--have spurred and formed the innovation activity, producing a program which is directly tailored to meet the immediate exigency. There should be little doubt, then, that the innovation's objectives will be met, given the appropriate time and resources. Interim shortfalls will prove transitory and be corrected as the full effect of the program takes hold. Or so the system proponent would have one believe.

But we know empirically that this is little more than a naive belief. Uncertainty pervades--indeed, characterizes--the technology innovation processes. Technological and procedural innovations can fail for any number of reasons, even when the causes are predictable and understood. Sometimes the extant technology cannot support the promise, either technically or economically, regardless of the resources invested.[1] Often times, when the technology is in hand, the institutional conditions are hostile.[2] And, finally, even the occasional felicitous junction of technology, process, and organizational receptivity leaves innovation and the expected diffusion wanting because of a change in the ambient conditions.[3]

From an analytic perspective, the success or failure of innovation is maddeningly paradoxical. In examining both technical and procedural innovations, researchers can consistently and accurately detail where and why failures have occurred. The reasons for specific failures are known. From these, policy recommendations have been drawn and innovative means of correcting shortcomings and generally promoting innovation are prescribed (Warner, 1974). Yet these too are just as failure prone. Closer examination reveals the reason: the particular circumstances that define the innovative context are so unique as to defy ready comparison with other innovation arenas, at least in a prescriptive mode. Evaluations of technical innovations are erroneously superimposed upon managerial innovations; little distinction is made between evaluating the technology and the technology delivery system. Wider samples based upon aggregate comparisons are usually statistically inconclusive (Ettlie, 1982). The "lessons" drawn from one set of innovations may be only problematically applicable to another, if at all; intra-industry comparisons are problematic and inter-industry transfers are dubious. Indeed, the solutions to the first organization's problems might actually be counterproductive in regard to the second.[4]

If the empirical record on innovation were not sufficiently discouraging, its intellectual underpinnings are equally (if not more so) deficient, perhaps even foreboding. Mowery (1983) has recently argued the inherent inadequacies of the economic theory of innovation, with special emphasis on Arrow's appropriability model. Political science has difficulty limning the interaction between the public and private sectors regarding innovation: where does one begin and the other end, especially as the two increasingly become inseparable partners?[5] What knowledge we do have, claim Nelson and Winter (1977: 41), "is in the form of congeries of semi-isolated facts, rather than a connected intellectual structure." In sum, while the total weight of research is surely hefty, "such theory as exists is poorly developed and insufficiently tested." (Mogee, 1980: 189)

But if innovation itself is difficult and quixotic, the demands for its benefits are even more difficult to allay or temper, especially in today's highly competitive commercial environments. The problem is hardly limited to specific firms or industrial sectors; the National Academy of Science warns that technological development must be

> ...among the highest national priorities of the nation. The United States must act now ... [because] the nation's capacity for technological innovation is vulnerable both from domestic weaknesses and from damaging practices of other nations. To lose out in this competition could be extremely damaging ....(Boffey, 1983)

Moreover, in a period of public and private recession and declining budgets, analysts are being urged to innovative means to salvage their organization's services (Walker and Chaiken, 1982). The insistent question, therefore, is not whether we can predict the ultimate success or failure of an innovation, nor (more fundamentally) if one should even attempt to innovate given the opportunity costs incurred as well as the direct attributable costs of failure. As an article of intellectual faith and policy, innovation activities will and should be encouraged and facilitated. The more immediate question addressed here is how does one evaluate on-going innovation activities, so that their progress can be most effectively managed, their end result most expeditiously obtained, and the final product most beneficially received by both sponsors and recipients.

This essay addresses the latter set of questions, leaving the first to those with a longer-range, more teleological cast. The particular concern here will be the evaluation of innovation *while it is on-going*, for it is during this period that critical choices must be made. A second emphasis will be on the policy implications of such evaluation activities, especially as they apply in the public sector. The objectives, then, are twofold: first, to encourage a more integrated understanding of innovation processes and, second, to propose specific ways in which these processes can be evaluated to improve our management of innovation midstream. These goals are based on two underlying assumptions: first, that it is preferable to evaluate innovation while it is evolving rather than permit it to continue unhindered to a possible final failure; and, second, that even in light of the discouraging, even dismal record, there are some optimistic possibilities, especially in the program evaluation literature, towards these objectives (Levine et al., 1981; Rossi and Freeman, 1982).

The focus of this essay is on the evaluation of individual programs, as it must be to describe and recommend evaluation requirements. Evaluating grand national strategies of innovation (e.g., the "reindustrialization" of the United States) is deliberately beyond its intended scope, although, taken in their aggregate form, the evaluation of individual programs could have implications for assessing the larger policies. More realistically, however, such strategies entail complexities surely beyond our current conceptual capabilities to prescribe and implement, let alone assess (Brewer and deLeon, 1983: Chap. 9) with any confidence in mid-course corrections.

McLaughlin tellingly describes the evaluation of innovation as "alchemy," arguing that our present "logic of inquiry is wrong. And preoccupation with scientism and with fixing our traditional evaluation paradigms scants what we do know.... One major challenge for evaluators, then, is epistemological: to develop new and valid ways of knowing." (McLaughlin, 1980: 46) The alchemy analogy is fitting, for at this stage of both the innovation and evaluation arts and crafts, the dogmatic adherence to scientific rigor or holistic approaches would be premature.[6] Rather, this essay deliberates what we already know in hopes of better utilizing existing knowledge and evidence. Thus, it adopts a deliberately heuristic spirit to provide directions and milestones towards the evaluation of innovation rather than a complete and dependable mapping.

Definitions, Examples, and Recommendations

One of the inherent problems of studying innovation is that it has assumed a numbing variety of divergent definitions. Furthermore, innovation can occur in a number of different arenas.[7] Most commonly, however, it can be categorized as either technical or procedural, although the distinction is hardly hard and fast.[8] The first generally refers to the research, development, and demonstration (RD&D) of a new technology, with the final success or failure of the technology assessed as a function of its commercial acceptance. Often this is dichotomized into product and process innovations. The procedural, or what some have termed behavioral, innovation deals less with technology and more with management and institutions. For example, Abernathy and Rosenbloom (1982) argue that both American and Japanese firms were presented with identical technological opportunities in the development and commercialization of consumer electronics but that the Japanese managerial (i.e., procedural) strategies provided them with the innovative edge over their U.S. rivals.

Evaluation refers to the assessment of a system, generally in a retrospective mode, in terms of its performance towards meeting specified objectives. For present purposes, evaluation is primarily defined as a means for assessing and managing the innovation process. Most of the work on evaluating innovation has been in the first category (i.e., technology), as analysts apply the standard criteria of technical feasibility, development time, and projected cost. There is, however, a growing awareness that while such measures are necessary, they are not sufficient for predicting innovation results (deLeon, 1982). Technical and economic measures must be matched with procedural metrics. In short, an expanded set of evaluative criteria must be proposed, tested, and applied if one is to assess with any confidence the innovative process while it is still on-going. Lacking this, the ability to correct the continuing process becomes more questionable than we can afford.

A first order consideration is our fundamental perception of the innovation process and how it shapes our expectations and analyses. As alluded to above, an organization or individual probably does not even begin R&D without expectant visions of a successful outcome; psychic and financial costs rarely permit such serendipitous excursions. For instance, financial return is typically cited as the primary cause underlying a firm's decision to risk investing in unproven technologies (Mansfield, 1968). Most innovation models and prescriptions are therefore based on and biased towards a premonition or presumption of success. Yet we know statistically that the great majority of attempted innovations ultimately end in failure or, at least, in achievements well below initial expectations. These failures usually are recognized only after heavy capital, institutional, and personal investments; early, easy terminations are the exception rather than the rule (Myers and Sweezy, 1978). The rare success (e.g., semi-conductors) seductively warps our view of the innovation process and its much less prevalent selection of new technologies. A SPRU (1972) study, which explicitly compared innovation successes with failures, couched its recommendations in terms of promoting the former as opposed to avoiding the latter. While this is perhaps understandable, the emphasis creates one critical disadvantage. To study mainly successes severely limits our knowledge of why things fail.[9] As failure is certainly the most frequent outcome, we are implicitly precluding the modal condition from our model. One needs to understand and compare both success and failure conditions to assess policies as they relate to the technology innovation process.

One of the major conceptual obstacles hindering the evaluation of R&D and many innovations is that their respective processes are often indiscriminately lumped and treated as an on-going, seamless progression in which scientific theory leads to applied technology leads to innovation and then diffusion. Rates of innovation may differ but the process is viewed as an integrated entity. Even admitting to an occasional glitch or hitch in the system, this depiction underlies much of the technology assessment philosophy and diffusion of innovation literature. It is, I propose, a detrimental myth, at best an infrequent phenomenon one should be hesitant to adopt as a policy-relevant model. Rather, I would strongly advocate the analytic *disaggregation* of the innovative process. It is more accurate and useful to identify discrete stages and judge them as a function of their own, more focused desiderata. However one may choose to demarcate the innovative process, it should be clear that different actors with different objectives and criteria play different roles during the innovation/diffusion drama. To force them simultaneously onto the same tableau might be theoretically plausible, but it ultimately disguises their individual contributions and--more to the point--our ability to evaluate their particular effects. For instance, the development and application of new technologies to urban environments has been a primary concern of the federal government, local municipalities, and many private sector firms.[10] Each has the same general objective--the delivery of better, more efficient services to a local clientele--but this blanket objective obscures more concrete, differentiated goals and criteria. In delivering improved health or personal security services, the federal government may be sponsoring new technologies under the broad mandate of public goods; the municipality might be reacting more to local rising costs (or decreasing revenues) and union demands; and the private sector participants could well be responding strictly to a profit motive. While none of these stands in forced opposition to each other, they are sufficiently divergent that to render an easy and accurate evaluation of an innovative technology and its accompanying delivery system is very difficult, especially when both are still evolving.

If, at the formative stages of innovation, evaluation criteria and milestones could be established for the various participants and periods, one would assuredly have a much better possibility of assessing the probability of a successful (or deficient) innovation. For example, early in the innovative activity, technical difficulties may prove the main obstacles and technical correctives accorded primary importance. Much later in the activity, the delivery of the RD&D product may be the critical impediment and procedural remedies may be more appropriate. Although both problems are common to the innovative process, each requires different measures and responses to be overcome.

The stage distinction might be important, but it is not easy to identify or resolve, either on a conceptual or operational basis. Even if one were to pose sharply distinguishable stages in theory, in practice they ebb and flow indistinguishably, one into the other. Institutional inertia, the uncertain, risky nature of R&D, and the problematic transitions from actor to actor reinforce the incremental nature of the successful innovation.[11] As McLaughlin (1980: 44) advises: "the process of implementation is heuristic--a process of learning and adjusting, rather than a process of installation." Furthermore, an undifferentiated, unswerving adherence to such milestones could undercut the innovative dynamic, for early deficiencies could conceivably be overcome in later stages if the RD&D were allowed to proceed; conversely, early optimism scarcely guarantees later fruition. There is, in a sense, an underlying innovation Darwinism implied here. Yet it should be clear that one cannot realistically assess innovation in process unless critical milestones are marked and observed and, furthermore, such winnowings are necessary as the costs of innovating (or not innovating) become increasingly great. It is not an easy balance to strike and maintain.

This first proposal is relatively specific in its practical applications. It calls for evaluation programs custom-tailored to their given innovation contexts: the technical and procedural palliatives which measure and apply to the development of an SST do not, *ipso facto,* apply to medical technology, and even less to behavioral or managerial innovations. My second suggestion is more concrete but could have a much more significant effect on our ability for innovation evaluation as a generic activity. Simply, the presently available data are inadequate for the current and escalating needs. Even if we admit to the shortcomings in the existing body of theory--which of course undermine the data definition and collection efforts--one is still struck by the paucity of reliable data that are comparable over a large number of cases. Branscomb (1983: 134) has observed that "the general perception is that you cannot...expect to find existing reliable, evaluated data, even on matters you know have been subjected to scientific research and publication." If we lack the ability to compare and contrast among incidents of innovation, then we are hardpressed to move up the evaluation learning curve. Too often one finds methodological sophistication being substituted for empirical validity; the end result, while perhaps epistemologically elegant, almost certainly does not satisfy the immediate public policy objectives and probably does not advance the state of the evaluation art because of the unreliable empirical inputs.[12]

While the observation "If you think our theory is bad, wait 'til you see our data" might be obvious to most, the mere recognition of the problem does not portend its quick relief for at least three reasons. First, as noted above, lacking some consensual theory of innovation, data requirements are difficult to formulate *a priori* (Downs and Mohr, 1976); one is confronted with the inevitable chicken and egg conundrum. Second, individual innovation cases can be so distinctive as to require very different measurement data. NASA-sponsored technology programs might legitimately be judged quite differently than those sponsored by the Department of Commerce. And finally, data do not grow naturally, as on analytic trees, to be harvested conveniently when succulent; an organic part of the program, they are rarely independently defined and cultivated for the sole analytic convenience of the evaluator. Too often, researchers "erroneously focus their attention on access and distribution rather than on the scholarship required to put data in a form in which users will dare rely on them." (Branscomb, 1983: 134) For all these reasons, the data problems loom imposingly against the evaluation of innovation while it is an on-going endeavor.

But this pessimistic litany is not necessarily a counsel of despair. Rather, it is more an admonishment of restraint, of more realistic and attainable objectives. We should not be blinded or obsessed by the immediate relevancy of case studies nor the long-range goal of general theory (Kaplan, 1964; Hirschman, 1976). At the current state of knowledge, usable typologies of innovation would be greatly beneficial and, I submit, directly applicable in the evaluation of innovations from a public policy perspective. Just as important, the development of such typologies would not only advance the state of the methodological art by highlighting what we know and do not know (i.e., some cells will be empty), but they are obtainable. For instance, one might wish to relate the interactions of "demand-pull" and "capability-push" R&D strategies with different "selection environments" (Nelson and Winter, 1977). If the development of such typologies could produce such results, then it would go a great way in alleviating the data problem presented above as well as making innovation evaluation more germane and accessible to decisionmakers.

This observation leads in a cumulative manner to a final consideration: the question of audiences and their objectives. This essay has an operating assumption that the evaluation of innovation is inherently more than an intellectual exercise, however stimulating that exercise might be. The implicit motivation is to improve the innovation process, that is, to provide both public and private organizations with a substantive understanding of "policy concerns [which] arise with respect to both the effects *on* the development in question and the effects *of* that development."[13] For this reason, the information and analysis must be made salient and accessible to innovators and the pertinent decisionmakers in a form they can use.

This observation is a logical deduction from the earlier two points in terms of three policy implications. First, evaluation must directly address the policy problem if it is to pass the essential relevancy test. This lends support to the tailored, specific approach as well as the movement towards a more generic typology. It is true that "The experimental paradigm, a useful tool in medicine and social psychology, where treatment is discreet and standard, cannot be fixed to fit the reality of planned change in social systems such as public education," (McLaughlin, 1980: 45) but it is possible that lessons gleaned from public education studies could be applicable to research on job training programs or other social programs. Second, to present persuasive policy recommendations, better data definition, collection, and analysis must be incorporated. This implies that "data grubbing" must be initiated from the very outset of the program so that they will fit the particular analytic requirements throughout the entire program for the complete set of participants. Lacking these, the analyst will have a difficult time convincing the policymaker of the accuracy and relevancy of the assessment, let alone what corrective measures should be adopted. Third, and perhaps most challenging, is the education of program managers as to the benefits of program evaluation and a greater appreciation of the more sophisticated tools that are available and relevant to their needs (Clarke, 1974). While this is hardly a straightforward assignment, it is essential if the evaluator's skills are to influence the innovation processes.

Some Final Observations

I have argued here that the evaluation of the in-process innovative dynamic is a difficult but increasingly necessary policy endeavor. This characterization applies in both its conceptual and practical aspects. If we are to gain intellectual and policy leverage on it, then it is imperative to move in discernible, attainable steps to limited, mid-range goals. The fact that they are limited in their immediate purposes does not mean that they are limited in their utility (deLeon, 1982).

Specifically, I proposed as first steps a disaggregation of the innovation process as a unit of analysis and the development of an innovation typology. These are, of course, not novel suggestions. What distinguishes them from existing models is their immediate and explicit connection to policy considerations, either in the private or, more likely, the public sector. These recommendations momentarily neglect a number of critical components delineating how one gets from here to there, such as the bases for structuring the typology, the criteria for differentiating within the innovative process, or the appropriate level of policy attention. But the purpose of this essay is more to lay out broad boulevards of policy-relevant evaluation rather than provide the building brick and mortar. Direction and purpose are as important as the final destination at this stage of our knowledge. A research corpus already exists which could be used as a starting point; Branscomb (1983) has proposed a "national science and technology data policy"; specific examples of RD&D stages can be cited which are at least illustrative and might serve as postulated models for stages in innovation.[14] In terms of extant methodologies and computational powers, we are already equipped well beyond our intellectual capabilities.

The need, I submit, is not for more--or even better--case studies of innovation nor for the development of more rigorous analytic tools, for neither directly addresses the necessary comparative, cross-industry innovation activities, nor how one evaluates the process. We are already case study glutted and methodologically muscle-bound. The more persistent needs are to restructure our intellectual and empirical understanding of innovation, largely using the existing evidence and tools. For instance, we should conceptualize innovation as a complex, nonlinear phenomenon[15] rather than treat it as a natural, monodirectional, rather systemic condition located somewhere on a learning curve. This approach suggests new ways of perceiving phenomena without demanding new data or methodological breakthroughs.

This essay has implied that there are positive, potential benefits to be derived from accurate, timely evaluations of innovation. In closing, we should also note the negative side of the same coin, the opportunity costs wasted by poor evaluation in terms of failing to terminate unwarranted projects until they become obvious. These "costs" can be economic (cost overruns), technological (the neglect of other, more promising technologies), or political (a public skepticism regarding technology[16]). As Porter and Rossini (1983: 728) remind us, "Society cannot afford to ignore the possible costs attendant on technological development, even as it continues to seek its bounties." For both the positive and negative reasons, we are thus moved to the difficult task of having to review (and possibly revise) the innovation drama while it is still in production, before the final denouement has occurred. This charter--although not easily fulfilled from an analytic perspective--and the conditions alluded to above should motivate the evaluator of the on-going innovation process with sufficient cause (some might claim urgency) to explore in greater detail some of the notions presented above.

ENDNOTES

[1] The liquid metal fast breeder reactor has yet to overcome its technical problems and fulfill the glowing commercial promise ascribed to it over 30 years ago. A recent analysis of LMFBR problems is Lanouette (1983).

[2] The difficulty experienced in establishing budgetary innovations such as PPBS and Zero Based Budgeting is explained by Wildavsky (1979, Chap. 6).

[3] Even with demonstrated technologies and generous government subsidies, the widespread diffusion of solar energy systems has been severely arrested, at least partially because of the public perception that the energy crises which characterized the 1970s were only passing phenomena.

[4] Identical education innovations have had very different effects when introduced in different school districts; see the multiple volumes authored by Berman et al. (1975).

[5] A comprehensive, if now somewhat dated, review of this relationship is Pavitt and Walker (1976).

[6] And unnecessary; see Kaplan (1964).

[7] Much of this is comprehensively reviewed by Mogee (1980).

[8] Both must be concomitantly considered; for instance, see Kidder (1981).

[9] deLeon (1978) offers some possible explanations for this emphasis.

[10] See Lambright et al. (1979) and Roessner (1979).

[11] These are stressed by Nelson and Winter (1977).

[12] A ready charge; for instance, see Acland (1979).

[13] The distinction is made by Porter and Rossini (1983: 728); emphasis in original. Cf. Lasswell (1971), who distinguishes between knowledge *in* and *of* the policy process.

[14] For example, Baer et al. (1977).

[15] This description was a major (although certainly not surprising) finding stressed by Nelson and Langlois (1983); also Mogee (1980).

16 A condition which should not be neglected; Miller (1983).

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